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Jared A. Ellias

ABSTRACT

Over the past 30 years, the majority of large firms that filed for bankruptcy did so in the US bankruptcy courts of the Southern District of New York and Delaware. Some believe these experienced courts dominate because their expertise makes bankruptcy more predictable. Critics dispute this explanation, arguing instead that “predictability” is a cloak for the true, self-interested motivation of the debtor’s managers, lawyers, and senior creditors who influence the debtor’s choice of venue. In this paper, I look for evidence supporting the views of the proponents and detractors of bankruptcy forum shopping in a large sample of market data. My results suggest that the market is better at predicting the outcomes of bankruptcy cases in New York and Delaware, consistent with the hypothesis that the law there is more predictable. I do not find evidence supporting the view that those courts are biased in favor of senior creditors.

1. INTRODUCTION

When large firms file for bankruptcy, they tend to do so in the US Bankruptcy Courts of the District of Delaware and the Southern District of New York. These two de facto national bankruptcy courts have jointly

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overseen more than 60 percent of all large bankruptcy cases in the past 25 years. While this equilibrium is now a well-entrenched feature of American bankruptcy law, it was not obvious that massive forum shopping would be the outcome when Congress passed the Bankruptcy Reform Act of 1978 (Pub. L. No. 103-394, 107 Stat. 4106). The new statute was flexible, providing troubled businesses the choice of the bankruptcy court in their state of incorporation, any jurisdiction that is home to major assets, or the firm's headquarters. However, as the act is federal law, the only advantages one jurisdiction might offer over another are geographic convenience, the body of judicially created precedent, and the quality and experience of the judges. The managers of large firms generally make the decision about bankruptcy venue in consultation with their lawyers and secured lenders. This paper is concerned with a much-debated question among academics and lawyers: why do these managers, lawyers, and lenders appear to prefer the bankruptcy process in two courts that are often far from a firm's headquarters? What has driven a generation of bankruptcy forum shopping?

A voluminous literature offers distinctly different answers to these questions. The Senate Judiciary Committee recently asked the Government Accountability Office to interview a large panel of bankruptcy lawyers and ask what leads them to file for bankruptcy in one venue over another (Government Accountability Office 2015). The lawyers' responses echoed the arguments of academic forum-shopping proponents (for example, Cole 2002; Ayotte and Skeel 2004; Skeel 2001), who hypothesize that firms are attracted to New York and Delaware because their combination of judicial expertise and developed legal precedent makes the bankruptcy process more predictable and efficient. Critics strongly contest this narrative and argue that "predictability" is a code word that provides a cloak for the self-interest of lawyers, managers, and secured lenders (LoPucki 2005). For example, a judge in Texas recently cast doubt on the value of judicial expertise, pointing out that the federal statute is the same in all jurisdictions. A better explanation, he implied, is geographic convenience for the East Coast lawyers and bankers who dominate corporate bankruptcy practice (see *In re Crosby Nat'l Golf Club, LLC*, 534 B.R. 888 [Bankr. N.D. Tex. 2015], in which the judge argued that forum-shopping proponents' explanations were "unconvincing"). Other critics see darker motives, alleging that managers and secured lenders prefer the destination courts because judges in those courts favor management and secured lenders at the expense of other stakeholders (LoPucki 2005).

This paper comes to the question of bankruptcy forum shopping with a new methodology and a hand-collected data set of 285 large bankrupt companies linked to the trading-price records of 1,049 financial contracts. Forum-shopping critics argue that bankruptcy lawyers have reasons to disguise their true motivation, which makes it difficult to understand what explains the current equilibrium. I sidestep this bias by looking for evidence supporting the predictions of forum-shopping proponents and critics in theoretically unbiased market data. I focus primarily on the main claim of bankruptcy-forum-shopping proponents: that predictable law and judges drive forum shopping. In a well-functioning market, the prices of a bankrupt firm's financial claims at the beginning of the bankruptcy process incorporate a large quantity of information that amounts to an unbiased estimate of the outcome of the bankruptcy process and the future value of the firm. If the proponents of forum shopping are correct, we would expect the market to be better at predicting bankruptcy outcomes in Delaware and the Southern District of New York than in other jurisdictions. Is it?

As firms are not randomly selected to different bankruptcy venues, I study the cross section of Chapter 11 debtors and look for evidence that supports the various theories that emerge from the bankruptcy-forum-shopping literature.¹ In my main results, I study squared investment returns for a hypothetical trader who buys a claim against a Chapter 11 debtor at the beginning of the bankruptcy process and receives the payoff at the end of the case. To illustrate the test statistic, consider a debtor who borrows \$100 from a bank. When the company falls into distress, distressed investors often buy claims like this one from prebankruptcy creditors at a discount to the face value of the debt. In this example, suppose the investor buys the claim for 50 cents on the dollar (\$50) and receives a payoff at the end of the bankruptcy process equal to 60 cents on the dollar (\$60). This investor earns a 20 percent return on her investment, or $(60 - 50)/50 = 10/50 = .2$. The squared return is $.2^2 = .04$. Importantly, if the investor sustains a 20 percent loss, the squared return would be $-.2^2 = .04$. Thus, the test statistic measures the absolute distance between the market's expectations for recovery at the beginning of the bankruptcy process and the discounted present value of the ultimate bankruptcy payoff, which I call the pricing deviation. All else being equal,

1. This paper is in the spirit of, for example, Barzuzza and Smith (2014) and Daines (2001), which examine the cross section of Delaware-incorporated firms.

a higher pricing deviation suggests that the market's pricing assumptions are relatively less accurate.

My results generally support the view that the market is able to form more accurate recovery expectations for the firms that reorganize in the two destination jurisdictions. Using both ordinary least squares (OLS) and quantile regression, I find that filing in Delaware or New York is associated with a statistically significantly lower pricing deviation than in other bankruptcy venues. This relationship is robust to controlling for firm size and industry, the lawyers advising the debtor, the duration of the bankruptcy case, changes in market conditions over the bankruptcy period, prepackaged or prenegotiated filings, and other potential confounding variables. Importantly, as further discussed below, the evidence is more robust for Delaware, as the New York venue dummy is not statistically significant in all specifications.

Unfortunately, an empirical research design that observes a selection process cannot eliminate the possibility that an omitted variable explains the results. I can, however, examine some of the more obvious confounding variables. For example, the market may simply be better informed *ex ante* about the types of firms that reorganize in the destination courts. There does not appear to be an obvious reason why this would be true. The firms in the sample are large, and there are no obvious confounding differences in the distribution of industries across the venue cohorts. Moreover, I examine a common proxy variable for market informedness in the literature—the rate of coverage by Wall Street research analysts—and I do not find evidence of a pattern that would explain the results. Alternatively, the firms that reorganize in Delaware and New York could simply be inherently more predictable firms. I examine proxy variables for firm-specific uncertainty used in other research, specifically prebankruptcy stock variance and cash-flow volatility, and I also do not find evidence of a systematic difference across the venue cohorts.

I examine other evidence to see how well the predictability hypothesis fits the data. In theory, the influence of predictable law and legal precedent should be strongest at the beginning of the bankruptcy process. As the firm moves through the process, the judge will issue the orders that should diminish uncertainty about the outcome, and the theoretical benefit of *ex ante* predictability should diminish. Consistent with this theoretical expectation, the observed pricing advantage of New York and Delaware persists in the early part of the bankruptcy case but disappears in the data as the case advances deeper into the plan of reorganization

process and as the firm prepares to emerge from bankruptcy protection. The results suggest that the market's observed advantage in pricing accuracy persists only during the period in which knowledge of the law and the judge would appear to matter the most.

In addition, I study the theoretical losers of increased predictability: holders of junior claims. As firms filing under Chapter 11 generally cannot pay their debts in full, junior claims such as unsecured debt and equity are often equivalent to out-of-the-money call options on the assets of the bankruptcy estate. Finance theory teaches us that call options are more valuable when underlying asset values are uncertain (see, for example, Merton 1974; Black and Cox 1976; Li 2013). Further, junior claimants have incentives to reduce their expected losses and extract holdup payments by using litigation to impose costs and uncertainty on senior creditors. In theory, junior claims are worth less when uncertainty is lower, and the bargaining leverage that junior claimants expect to acquire through litigation might be reduced if the law has fewer ambiguities to exploit and the judge has the experience to filter weak claims. As theory suggests, I find that out-of-the-money claims of Delaware-venued bankrupt firms appear to be worth relatively less at the beginning of the Chapter 11 process than claims of firms reorganizing in venues with less experienced courts, controlling for heterogeneity in capital structure, market conditions, and the ultimate payoff.² Importantly, as further explained below, this does not appear to be the result of a transfer of value from senior creditors. However, I do not find evidence of the same relationship for the New York-venued sample.

In Section 4, I consider some of the predictions of detractors of forum shopping. I examine the market price of senior claims at the end of the bankruptcy process, and I do not find evidence that bankruptcy judges in the destination venues disproportionately approve plans of reorganization that transfer value to senior creditors from junior claimants. Clearly, there are other channels through which a pro-secured-creditor bias (or a pro-management bias) could be expressed, and further research is needed. While this paper offers suggestive evidence supporting the views of the proponents of bankruptcy forum shopping, I cannot rule out the possi-

2. In the language of option-pricing theory, the value of the out-of-the-money claim theoretically increases in the volatility of the bankruptcy case. My results suggest that the greater certainty of more experienced courts effectively reduces the volatility of the bankruptcy case, and the pricing consequences appear to be consistent with what option-pricing theory would predict.

bility that the predictions of both the proponents and detractors of forum shopping have merits. I also cannot say that the firms that reorganized elsewhere would have been better off filing for bankruptcy in a destination venue. The paper's main conclusion is that the market appears to be better at predicting the outcome of the bankruptcy process in more experienced bankruptcy courts and that this correlation is robust to controls and supported by other evidence.

This paper proceeds as follows. Section 2 introduces the sample and provides summary statistics. Section 3 presents the research design and main results. Section 4 concludes with a brief discussion of recurring proposals in Congress to reform the bankruptcy statute and end forum shopping. The results suggest that venue reform might increase the uncertainty of the bankruptcy system, at least in the short term, while the market adjusts.

2. THE SAMPLE

At a high level, my methodology tests how well the market predicts the outcome of the bankruptcy process. When large firms file for bankruptcy, their claims—both debt and equity—continue to trade in the marketplace.³ There is no readily available source that links the trading records of bankrupt firms to a data set of bankrupt companies. Accordingly, I compiled a large data set by hand. My final sample consists of 285 firms that filed for bankruptcy between 2001 and 2012 linked to the pricing records of 1,049 financial contracts, including bank debt, bonds, and equity.⁴ One attribute of the sample worth noting is that it combines

3. A secondary market also exists in the trade claims (the claims owed by the debtor to trade creditors, as opposed to financial creditors) of bankrupt firms (Ivashina, Iverson, and Smith 2016), but that trading market is thought to be relatively small compared with the much larger claims of financial creditors and is largely outside the scope of this paper.

4. My source for bond data goes back only to 2002, so I do not have bond pricing information for 2001, although many of the bonds for the 2001 sample enter the data set when the bankruptcy case advanced into 2002. The bond data were particularly challenging to study. I cleaned the data as described in Dick-Nielsen (2014), but there is no authoritative list matching bond contracts to the unique bond identifiers such as Committee on Uniform Security Identification Procedures numbers. I relied on the Mergent Fixed Income Securities Database's list of bonds to make my initial list of potential bond matches, but those data are very incomplete. For many bond contracts, I was able to identify the matching pricing record only after a significant investigation of the firm's Securities and Exchange Commission disclosures. It is possible that a more complete list of bonds might identify more candidate firms for the sample, but none exists to the best of my knowledge.

different types of pricing data: dealers' quotes for corporate loans with records of actual trades for bonds and equity. To ensure that this does not bias the results, I check my main regression results on subsamples restricted to loans, bonds, or equity. I provide detailed information about the sample's construction and discuss the possibility of bias in greater detail in the online appendix.

To the best of my knowledge, the sample contains all large companies that filed for bankruptcy reorganization during the sample period with debt that traded in public or private markets and for which prices could be observed.⁵ Importantly, the sample does not include firms that announced on their petition date that Chapter 11 would be a vehicle for a sale or liquidation (typically in a so-called Section 363 sale) outside of the statutory process for a plan of reorganization. The firms in my sample all announced on the petition date that Chapter 11 would be a vehicle for a business reorganization and that, if a sale was a possibility, it was not yet a certainty. I made this choice for three reasons. First, trading volume is significantly lower for firms that announce on the petition date that they plan to sell the firm, which suggests that the market for the claims of those firms may be different. There is, for example, less of a role for distressed hedge funds in bankruptcies in which the focus is on marketing the firm's assets, and there are fewer conflicts over governance. Second, the theory assumes that the market's theoretically improved ability to predict bankruptcy outcomes will be driven by knowledge of law, and the law surrounding the use of Section 363 to sell a firm's assets is largely the same deferential standard in all jurisdictions. Third, some of the analyses below follow the firm through different parts of the bankruptcy process, and excluding firms that did not intend to go through the bankruptcy process from the overall sample provides for a more consistent sample.⁶

Table 1 summarizes the venues, and Table 2 provides summary statistics on the financial characteristics, industry, and public registration status of the sample's firms. As Table 2 shows, Delaware and the non-destination courts seem to administer similarly sized debtors, while the firms filing in the Southern District of New York are significantly larger, consistent with its status of venue of choice for the largest cases such as

5. The online appendix discusses issues with missing data.

6. As further discussed below and shown in online appendix Table OA9, the results are qualitatively similar when the regression model with full controls is estimated over the full sample of claims including Section 363 cases, with additional dummy variables that capture aspects of the sales process.

Table 1. Venues of Firms in the Sample

Bankruptcy Court	Frequency	%
Delaware	121	42.46
Southern District of New York	79	27.72
Northern District of Illinois	8	2.81
Northern District of Texas	6	2.11
Eastern District of Michigan	5	1.75
New Jersey	5	1.75
Nevada	5	1.75
Southern District of Texas	5	1.75
Northern District of Georgia	4	1.40
Southern District of Ohio	4	1.40
Other ^a	43	15.10

^a Consists of 26 courts that handled three or fewer cases each.

Lehman Brothers and General Motors. In addition, as Table 2 shows, the sample is relatively balanced in terms of industry between Delaware and the other courts, with Delaware administering slightly more cases involving manufacturing businesses.

For each firm in the data set, I gathered information about the underlying bankruptcy from the firm's court docket. In addition to basic financial information, I identified the venue, the judge, the debtor's law firm, and other information. I also identified important hearings and orders in the bankruptcy process and any objections filed by creditors to identify dates on which the judge's actions would be particularly important.

3. RESEARCH DESIGN AND RESULTS

My research design is motivated by the possibility that more experienced bankruptcy courts might make it easier for the market to predict the outcome of bankruptcy cases, which would improve observed price accuracy. If, as Cole (2002) suggests, bankruptcy law in Delaware (and New York) is more predictable than in less experienced courts, presumably that might result in improvements in the market's ability to price the claims of the bankrupt firms that reorganize in those venues. Investors, for example, will not have to weigh probabilities foreclosed by Delaware or New York precedent. They might be able to project the administrative costs of bankruptcy with greater certainty. The debtor's case may present

Table 2. Summary Statistics

	Delaware		Southern District of New York		Other	
	Mean	Median	Mean	Median	Mean	Median
Financial characteristics:						
Funded debt (\$ millions)	1,031	495	4,258	1,028	989	434
Petition assets (\$ millions)	1,630	3,009	7,533	1,579	1,745	672
Secured debt (\$ millions)	331	205	677	379	370	163
Secured debt as a percentage of assets	.44	.41	.35	.3	.43	.39
Industry:						
Consumer durables, nondurables, and retail	.34		.23		.31	
Manufacturing, energy, and utilities	.31		.19		.18	
Business equipment	.12		.27		.13	
Health care and medical equipment	.03		.05		.04	
Other ^a	.2		.27		.35	
Prebankruptcy SEC registration:						
Registered public equity	.45		.61		.56	
Registered public bonds	.76		.82		.79	
Registered equity or bonds	.83		.86		.89	

Note. Funded debt consists of all debt owed pursuant to financial contracts (as opposed to trade debt or tort debt). SEC = Securities and Exchange Commission.

^a Includes construction, hospitality, and finance.

issues that were recently resolved by the same judge in a different case, which would allow the market to predict with greater certainty how the judge might rule. The intuition is that having a Delaware or New York bankruptcy filing as opposed to a St. Louis bankruptcy filing communicates more information to the market, which results in improved observable forecasting price efficiency.⁷ Thus, I introduce my hypothesis.

Hypothesis. All else being equal, the prices of claims of firms reorganizing in Delaware and the Southern District of New York on the petition date should be closer to the ultimate bankruptcy payout than firms reorganizing in venues with less experienced courts.

3.1. Research Design

To test the hypothesis directly, I calculate squared returns to measure pricing accuracy.⁸ At a high level, squared returns are simply the squared value of the traditional formula for calculating investment returns. More formally,

$$r_c^2 = \left(\frac{\text{bp}_c - p_c}{p_c} \right)^2,$$

where r_c^2 is the squared value of the buy-and-hold return of an investor who bought claim c at the beginning of the bankruptcy process and held it until receiving the bankruptcy payoff bp_c . To reduce the likelihood that the results are biased by outlying pricing observations, I define p_c as the weighted average transaction price of all observed trades in the 30-day window around the filing of the bankruptcy petition.⁹ Similarly, the bankruptcy payoff bp_c is the average trading price of all observed trades in the month around the confirmation of the plan of reorganization. Throughout, I discount bp_c to the present value as of the petition date to account for the time value of money.¹⁰

7. An untested assumption is that all of the other courts are the same. This could be wrong. There could be, for example, another jurisdiction (or judge) with a profile similar to Delaware's or New York's. I lack a theoretical basis to identify what court that might be, as there is no obvious third choice in the data.

8. As Table OA5 shows, the results are the same if I instead use the absolute value of an investment return as the test statistic.

9. I use the average price across all weighted trades in the trading window or the quoted price for my loan data. The trading window is defined as the 15 days prior to and the 15 days following the petition date. As Table OA6 shows, the results are qualitatively similar if I instead use windows of 0, 1, 2, 3, 5, or 10 days around the petition date.

10. I follow Li and Zhong (2013) and use the 1-year yield on treasury bonds as the risk-free rate. The results are not sensitive to different treasury bond maturities. In prac-

Eberhart, Moore, and Roenfeldt (1990, p. 1465) suggest that the market value of a claim against a bankrupt firm “should reflect the amount [claimholders] expected to receive upon reorganization, discounted to account for the expected delay in payment and adjusted for risk.” Stated more explicitly, in a frictionless market, the market price of claims at the beginning of the bankruptcy process should be equal to the discounted value of the expected bankruptcy payoff. This will usually be the proceeds of a sale or a claimant’s share of the value of the reorganized firm, with value in both cases distributed according to the absolute-priority rule. In a frictionless market, choice of venue should not help the market predict the value of the claims at the end of the bankruptcy process. However, if I allow for bankruptcy costs and other frictions, the venue and the bankruptcy judge can impose uncertain costs in the forms of delays and variations in courts’ decisions. This uncertainty could present itself as deviation from what one would expect in a frictionless market, which would reduce the accuracy of forecasts of future recoveries.

Thus, the test statistic aims to capture this pricing deviation, and it can be thought of as a measure of pricing accuracy.¹¹ To illustrate, consider an investor who purchases a claim for \$10 that produces a discounted payoff of \$15. This investor receives a 50 percent return on her investment; r_c^2 for this investor would be $[(15 - 10)/10]^2 = .5^2 = .25$. Importantly, if this investor sustains a 50 percent loss, r_c^2 for this investor would be $[(5 - 10)/10]^2 = -.5^2 = .25$. In a frictionless and well-functioning market, the investor would sustain neither a gain nor a loss, and r_c^2 would equal 0 since the payoff has been discounted to present value. Realistically, the market likely uses a different discount rate, so I would expect r_c^2 to usually be larger than 0. As all values of r_c^2 are positive, r_c^2 measures the distance of the return from the ideal of perfect pricing accuracy.

The main advantage of this test statistic is that it facilitates straightforward interpretation of signed coefficients in a regression model. A vari-

11. In a contemporaneous working paper that studies the accuracy of court-ordered valuations, Demiroglu, Franks, and Lewis (2015) use a similar test statistic, the absolute value between court valuations and subsequent market evidence. I adopt their terminology of referring to that test statistic as measuring “error” and “deviation.”

Table 3. Pricing Deviation

	N	Mean	Median	SD
Venue:				
All claims	1,049	1.64	.08	8.09
Delaware	378	2.29	.09	10.38
Southern District of New York	403	1.45	.08	5.59
Other	268	.98	.07	7.55
Type of claim:				
All secured claims	514	.53	.01	3.24
First-lien secured claim	205	.23	.01	.93
Second-lien secured claim	47	.61	.1	1.71
Secured claim with lien on specific asset	163	1.14	.03	5.54
All unsecured claims	343	3.37	.39	11.62
Senior unsecured bonds	218	2.73	.48	6.57
Subordinated unsecured bonds	64	5.18	.25	20.9
Equity	137	1.78	.65	10.6

able that is negatively related to the pricing deviation implies that it is associated with the magnitude of that squared return being smaller, all else being equal, and that it is associated with more accurate pricing as of the petition date. Conversely, a positive coefficient implies less accurate pricing. Table 3 summarizes the squared returns of the claims that are traded at the beginning of the Chapter 11 process.

Two important patterns emerge from Table 3. First, attributes of the financial contract, such as a claim's priority, appear to be important. For example, predictably, secured claims appear to be priced relatively more accurately than unsecured claims and equity.¹² Table 2 summarizes some aspects of the capital structures of the firms in the sample, and there do not appear to be enormous differences in ex ante capital structures when sorted by venue. However, Table 3 demonstrates that it is important to control for observable attributes of the financial claims to test the hypothesis. Second, Table 3 shows that the means and medians are very

12. In the specifications below, I generalize financial contracts with variables that measure relative seniority (through the percentage of the capital structure senior to the claim) and collateral. I do not follow the traditional approach in the literature of using dummy variables for characteristics of claims like security and seniority because my approach appears to be more precise, and introducing additional dummy variables would raise problems of multicollinearity. The results are similar if I rely on dummy variables instead, as Table OA7 shows. In building the sample, I analyzed the capital structure at the contract level, which provides for a more generalizable set of control variables than in previous work.

different, which points to the wide dispersion in the data and the need to account for outliers that could bias the results.¹³

Accordingly, I account for the problem of outliers in the sample in two ways. First, I trim the outlying 2 percent of observations from the results to assure that no observed relationship is unduly influenced by outlying observations. The results are qualitatively similar if I analyze the full sample, which suggests that outliers are not a substantial driver of the results.¹⁴ Second, I use estimation methods that are more robust to outliers. In particular, I follow Billett et al. (2015), which confronts the same problem of skewed debt returns and supplements OLS models with quantile regression models. Quantile regression models estimate the conditional median of the observed sample, as opposed to OLS models, which estimate the conditional mean. As Billett et al. (2015) explain, quantile regression models are often used to test the robustness of OLS results because they are less sensitive to the presence of outliers.

3.2. Results

3.2.1. Baseline Price Accuracy Result. My basic model of the pricing deviation for each observed claim c is

$$\ln(r_c^2) = \alpha + \beta \text{Delaware}_c + \beta \text{SDNY}_c + \delta \text{Controls}_c + \varepsilon. \quad (1)$$

The dependent variable r_c^2 is the observed pricing deviation, Delaware_c and SDNY_c are dummy variables for the venue in which the claim is being reorganized, and Controls_c is a vector of variables that account for other possible determinants of r_c^2 , some relating to observable characteristics of firms and claims and others capturing elements of the bargaining environment.

A potential bias could be imparted by the fact that my unit of observation is the financial contract issued by each debtor in the sample. While some debtors have one contract traded at different levels of claim

13. Importantly, as I show in Table OA8, I use one- and two-sided t -tests to study the distribution of petition date prices across the three venue cohorts of firms, and I fail to find statistically significant differences that the samples' prices are drawn from different distributions.

14. When the models specified in Table 4 are estimated instead over the full sample using both ordinary least squares (OLS) and quantile regression, the Delaware and New York venue dummies remain negatively and statistically significantly associated with the pricing deviation with full control variables, and the magnitudes of the coefficients are similar. Table OA9 shows the regression results on the full sample of claims, including outliers (models 1 and 2) and cases in which management announced the intention to use Chapter 11 to conduct an auction on the first day of the bankruptcy case, chiefly through a Section 363 sale (models 3 and 4).

priority (for example, a secured term loan, a senior unsecured bond, and a subordinated bond), other debtors have multiple contracts trading with the same level of priority. For example, an industrial firm might have borrowed money by issuing several unsecured bonds with different maturity dates, which bankruptcy law converts into unsecured claims of equal priority. To avoid allowing this to bias the sample, I calculate a single value for r_c^2 for every level of payment priority against each firm in the sample. So, in the case of an industrial firm with multiple unsecured bond contracts outstanding, I take the mean trading price of all traded bond claims within the applicable trading window and include it in the regression sample as a single observation.¹⁵

Table 4 presents estimates of the relationship between bankruptcy venue and pricing accuracy using different estimation methods. In model 1, I use OLS regression with fixed effects for industry and year on the pricing deviation and find a negative and statistically significant association for the Delaware venue dummy. In model 2, I include plausibly exogenous controls such as firm size and claim and capital structure variables. In this model, the New York venue dummy variable becomes negatively and statistically significantly associated with the observed pricing deviation. In Table OA10 of the online appendix, I verify that only one additional control variable—the amount of debt owed by the firm, a proxy for firm size—is needed to reveal the observed negative association between the New York venue dummy and the pricing deviation.

It is possible that these results might be driven by differences in the distribution of financial contracts across the bankruptcy venues.¹⁶ To try to control for those differences, I employ three additional control variables. First, I control for the claim's relative position in the capital structure by calculating the percentage of the debt capital structure that has a higher priority than the claim.¹⁷ This number ranges from 0 (generally, for senior debt claims) to 1 (for equity). I also control for whether the

15. As models 5 and 6 of Table OA9 show, the results are the same if I use financial contracts as the unit of analysis instead of one observation per traded claim with a common level of priority against the debtor.

16. Table OA11 summarizes the distribution of types of financial contracts in the regression sample.

17. In Table OA12, I look for evidence that the influence of Delaware or New York law might increase (or decrease) in the relative seniority of the claim. I add interaction terms for the capital structure variables (percentage of capital structure senior to the claim and the dummy for lien on a specific asset) and the venue dummies to the models in Table 3, and the resulting interaction terms are not statistically significant. This result does not support the view that the influence of Delaware or New York law increases or decreases in the relative seniority of the claim.

Table 4. Determinants of Pricing Deviation

	Ordinary Least Squares					
	(1)	(2)	(3)	(4)	(5)	(6)
Delaware	-1.045*	-.894*	-1.249**	-1.365**	-.669**	-.737*
	(.471)	(.420)	(.412)	(.403)	(.245)	(.358)
SDNY	-.423	-1.135**	-.906*	-.550	-1.030**	-.781*
	(.426)	(.409)	(.402)	(.442)	(.328)	(.374)
Log-funded debt		.495**	.279+		.447**	.387**
		(.147)	(.144)		(.091)	(.104)
Private equity owned		-1.369**	-.918*		-.868**	-.547
		(.481)	(.457)		(.282)	(.379)
% Firm capital structure senior to claimant		3.040**	3.065**		3.248**	3.200**
		(.359)	(.362)		(.253)	(.286)
Lien on specific asset		-.917+	-1.121*		-.537	-.612
		(.523)	(.510)		(.363)	(.386)
Prepackaged bankruptcy		-2.490**				-2.122**
		(.814)	(.814)			(.651)
Prerenegotiated bankruptcy		-.476				-.540+
		(.449)	(.449)			(.293)
Log squared return of comparable firms		.022				.024+
		(.019)	(.019)			(.012)
Log observed case duration		.386				.345+
		(.250)	(.250)			(.178)
R ² or pseudo-R ²	.11	.27	.35	.07	.21	.25
N	604	604	575	604	604	575
Debtors	282	282	270	282	282	270
Law firm fixed effects	No	No	Yes	No	No	Yes

Note. The dependent variable is the logged squared return that an investor who bought the claim on the petition date would have realized if she had sold the claim at the end of the bankruptcy process, based on the observed market value of the claim. The bankruptcy payoff is discounted to present value as of the petition date. Industry fixed effects are the Fama-French 38-industry classifications. Standard errors clustered at the firm level (ordinary least squares models) and robust standard errors (quantile regression models) are in parentheses. All regressions include industry and year fixed effects.

+ $p < .1$.

* $p < .05$.

** $p < .01$.

contract gave the creditor a lien on one of the firm's assets, such as real estate, inventory, a plant, or an airplane, which might be associated with a higher recovery rate than a lien that relies on more nebulous going-concern value.¹⁸

In model 3, I add additional control variables to test the robustness of the result. Bankrupt firms file for Chapter 11 with different levels of readiness to proceed through the bankruptcy process. In a prepackaged bankruptcy, management has reached an agreement with all of its creditors and has already solicited a vote on the plan of reorganization. In a prenegotiated bankruptcy, management has reached an agreement with some, but perhaps not all, of its creditors, and no vote has been solicited. I also control for the squared return of firms in the same industry over the bankruptcy period, the length of the bankruptcy case, and the debtor's law firm.¹⁹ As models 1–3 show, after controlling for these additional characteristics of the underlying business, the bankruptcy, and the financial contract, the market appears to price the claims of debtors reorganizing in the two experienced bankruptcy courts more accurately. In models 4–6, I reestimate the same models using quantile regression, and the results are similar.

As a further robustness check, I estimate the OLS regression model on subsamples of the data and with alternative control variables, and Table 5 shows those results. In models 1, 2, and 3, I estimate the full model (without the capital structure variables) on the bond, equity, and loan data, respectively. The Delaware dummy is negative and statistically significantly associated with the pricing deviation in these subsamples, but the New York–venue dummy is statistically significant only when analyzing the cross section of bond claims. In models 4 and 5, I use alternative control variables for firm size: prebankruptcy earnings before interest, taxes, depreciation, and amortization (EBITDA) and EBITDA-to-debt ratio, and the results remain similar to those in Table 4.

In sum, I find a persistent and statistically significant negative relation

18. I analyzed the capital structure of each sample thoroughly, so this variable includes cases in which the creditor has a claim against a subsidiary corporation with one of the firm's assets (that is, the land for a case involving a hotel) and cases in which the loan was structured by conveying a lien on a specific asset to the lender.

19. I control for law firm because the market's knowledge of the debtor's law firm (for example, familiarity with the tendencies and biases of particular law firms or their work in other bankruptcies) might help the market form more accurate expectations about recovery. Conversely, outcomes for an unknown firm might be harder for the market to predict.

Table 5. Robustness Tests of Determinants of the Pricing Deviation

	(1)	(2)	(3)	(4)	(5)
Delaware	-1.443*	-1.207*	-1.245*	-1.863*	-1.985*
	(.655)	(.607)	(.620)	(.806)	(.849)
SDNY	-1.386*	-.654	-.550	-1.636*	-1.812*
	(.548)	(.532)	(.640)	(.811)	(.843)
Prepackaged bankruptcy	-4.916**	.363	-5.144**	-.463	-.537
	(1.051)	(1.034)	(.822)	(1.667)	(1.675)
Prenegotiated bankruptcy	-1.607**	1.494 ⁺	-1.436*	-.375	-.420
	(.465)	(.800)	(.600)	(.988)	(.990)
Lien on specific asset	-1.870*		1.320 ⁺	-2.959*	-2.951*
	(.904)		(.764)	(1.182)	(1.189)
Private equity owned	-.603		-.161	-1.570 ⁺	-1.600 ⁺
	(.679)		(.515)	(.926)	(.935)
Log squared return of comparable firms	.008	.043	.058 ⁺	.018	.014
	(.024)	(.039)	(.031)	(.042)	(.041)
Log EBITDA				.230	.334
				(.265)	(.262)
% Firm capital structure senior to claimant				2.941**	2.916**
				(.596)	(.599)
Log observed case duration				1.641*	1.525*
				(.691)	(.678)
Log EBITDA-to-debt ratio					-.400
					(.450)
R ²	.53	.40	.36	.45	.45
N	348	128	444	256	256
Debtors	140	128	185	108	108
Sample	Bonds	Equity	Loans	PACER	PACER

Note. Results are from ordinary least squares regressions, with standard errors clustered at the firm level in parentheses. The dependent variable is the logged squared return that an investor who bought the claim on the petition date would have realized if she had sold the claim at the end of the bankruptcy process, based on the observed market value of the claim. The bankruptcy payoff is discounted to present value as of the petition date. The bonds sample includes all bonds in the data set, the equity sample includes all common equity, and the loans sample includes all secured loans. The Public Access to Court Electronic Records (PACER) sample is the sample of claims with full PACER data (generally, cases filing for bankruptcy in 2004 or later) that disclosed prebankruptcy earnings before interest, taxes, depreciation, and amortization (EBITDA) or for which it could be obtained from contemporaneous filings with the Securities and Exchange Commission. All regressions include industry (Fama-French 38-industry classifications), year, and law firm fixed effects.

⁺ $p < .1$.

* $p < .05$.

** $p < .01$.

between reorganizing in the two experienced bankruptcy courts and the observed pricing deviation. Stated differently, the market appears to be better at predicting the ultimate value of the claims of firms reorganizing in more experienced bankruptcy courts, although the relationship is more robust for the Delaware-venued sample of claims than for the New York sample.²⁰

3.2.2. The Challenge of Omitted-Variable Bias. It is important to qualify these results by returning to the limitations of the research design. The cross-sectional results suggest that the market can better predict the outcome of the bankruptcy process when firms reorganize in more experienced courts. However, the research design does not allow for a strong causal claim, as the firms that file for bankruptcy in Delaware or New York do not do so randomly. There could be an omitted variable that drives the results.²¹ Ideally, I would test the hypothesis by randomly transferring firms that filed for bankruptcy in the destination courts to less popular jurisdictions and randomly transferring firms from less experienced courts to the destination venues.²² Virtually every firm could reach a destination court with a creative bankruptcy lawyer, even if it had minimal contacts prior to bankruptcy.²³

The most obvious possible confounding issue is that the market simply knows more about the cohorts of firms that file for bankruptcy in New York and Delaware than the firms that file elsewhere. The accounting literature refers to the level of knowledge that the market has about a firm

20. In Table OA13, I show that the results are qualitatively similar if I omit the cases in which unsecured creditors prosecuted significant avoidance actions, which suggests that the differential distribution of claims subject to avoidance is not what appears to drive the results in Table 3.

21. LoPucki and Doherty (2002) examine a sample of public company bankruptcies from 1991 through 1996 and conclude that their cohort of firms filing for bankruptcy in Delaware was not different, on the basis of observable financial characteristics, from the cohorts filing in other venues.

22. The sample does contain four cases in which a judge in Delaware or New York granted a creditor's motion filed early in the bankruptcy case transferring the case to a less experienced court. The moving party was never a financial creditor—it was the US trustee, arguing that venue was improper; a government official; or a union that wanted the firm to reorganize in a venue closer to home. While caution is advised in interpreting the trading activity surrounding these nonrandomly selected transfers, the average pricing deviation of the claims of those firms appears to have increased 1.3 percent when comparing the day after the transfer order with the day before it.

23. Patriot Coal, for example, created new New York–domiciled subsidiaries with minimal assets to use as a basis for filing for bankruptcy in the Southern District of New York. These subsidiaries were created essentially on the eve of bankruptcy.

as market informedness. There do not appear to be any obvious reasons why market informedness would differ across the three cohorts of firms. As Table 2 shows, the majority of firms filing for bankruptcy in Delaware (83 percent), the Southern District of New York (86 percent), and the less experienced courts (89 percent) were registered public filers with the Securities and Exchange Commission (SEC) prior to bankruptcy. Further, continued compliance with SEC disclosure obligations does not appear to correlate with venue. For the Delaware cohort, 35 percent of firms continue to file SEC disclosures for the year preceding bankruptcy, as compared with 39 percent of the Southern District of New York cohort and 36 percent of the cohort of other courts.

A common approach to market informedness in the literature is to examine proxy variables, such as the number of Wall Street analysts covering the firm (see, for example, Chae 2005). Research analysts expend resources producing detailed reports on firms, which leads many to believe that firms covered by research analysts will have more accurate prices. To investigate the distribution of coverage by analysts across the three cohorts of firms, I examine the historic data on such coverage from the Institutional Brokers' Estimate System to see if it systematically varies across the three cohorts of firms.²⁴ I find that analysts appear to cover about 40 percent of the sample's firms in the year prior to bankruptcy, with the rate of coverage being significantly higher in the Southern District of New York (49 percent of firms) than in Delaware (36 percent) or the cohort of other courts (32 percent). Similarly, the median number of analysts covering New York-venued debtors is higher (4.5) than that covering firms that filed for bankruptcy in Delaware (2) or in the less experienced bankruptcy courts (3). Overall, the results suggest that the relatively larger firms that choose to reorganize in New York are better covered by analysts but that there is no statistically significant difference in the rate of coverage or the number of analysts covering the similarly sized firms that file for bankruptcy in Delaware or in other courts.²⁵

Alternatively, some researchers have used the bid-ask spread as a proxy for the level of information in the market. The bid-ask spread is the difference between the offers to buy a claim in the marketplace (the

24. Table OA14 shows these results.

25. As Table OA14 shows, the results are qualitatively similar if I examine the mean number of analysts instead of the median or if I use the year of the bankruptcy petition instead of the year prior to bankruptcy. In addition, Table OA15 shows that the results in Table 3 do not change if I include an analyst-coverage control variable in the regression models.

bid) as compared with the offers to sell the claim (the ask). Some portion of the bid-ask spread is thought to reflect informational asymmetry between informed and uninformed traders, as uninformed traders demand a risk premium when they are at an informational disadvantage. Some research shows that reforms like accounting disclosures that increase the informedness level of the market reduce bid-ask spreads (for example, Greenstein and Sami 1994), which suggests that a higher bid-ask spread might indirectly reflect a marketplace that is less well-informed and higher informational asymmetry between traders. In Table OA16, I examine the observed bid-ask spread for the Loan Syndications and Trading Association loan data and estimated bid-ask spreads for the bond and equity data prior to bankruptcy filing, and I do not find evidence suggesting that the market is systematically less well-informed about the underlying businesses of the firms reorganizing in less experienced courts.²⁶

However, none of this indirect evidence of market informedness bears squarely on another potential confounding variable: the level of firm-specific uncertainty. It may be the case that the market is equally well-informed about all three groups of firms, but the value of firms reorganizing in less experienced courts could be more uncertain than that of firms reorganizing in Delaware or New York for reasons that have nothing to do with bankruptcy law or the experience of the judges and are not otherwise captured by the control variables in Table 4. If the uncertainty surrounding the value of a firm is greater, the market will have a more difficult time forming accurate expectations about recovery from bankruptcy. Unfortunately, uncertainty, like market informedness, cannot be observed directly.

Accordingly, I use two methods from the literature to examine the possibility that the uncertainty regarding the three cohorts of firms is different. First, Bloom and Van Reenen (2002) proxy for firm-specific uncertainty by measuring the variance of the firm's publicly traded common stock returns. I follow their methodology and examine the variance of the firms with public equity that trade actively in the week, month, year, and 2 years prior to bankruptcy. As Table OA17 shows, I do not find evidence of a systematic difference between the three groups of firms. Cash-flow volatility is an alternative measure of uncertainty. I follow Rountree, Weston, and Allayannis (2008) and use the standard deviation of firms' revenue, scaled by their assets, as a proxy for cash-flow volatility. I am

26. I estimate the bid-ask spread for the bond and equity data using the method in Corwin and Schultz (2012).

able to calculate the prebankruptcy cash-flow volatility for the firms that continued to file disclosures with the SEC that covered the year prior to bankruptcy. As Table OA18 shows, this proxy also does not indicate any reason to worry that the three cohorts of firms are somehow different in an unobserved way.

3.2.3. Pricing Accuracy through the Bankruptcy Process. Beyond the main results, I look for additional evidence consistent with the view that predictability drives the market's observed advantage in pricing accuracy for New York- and Delaware-venued debtors. If the correlation documented above does, in fact, reflect the impact of predictable law and courts, we might expect that relationship to disappear as the firm moves through bankruptcy. Chapter 11 is a structured bargaining process, and management negotiates with creditors against the backdrop of intermediate judicial orders. Uncertainty in the case resolves as the case moves forward. To examine how uncertainty might change over the case's duration, I calculate the pricing deviation for each cohort at three additional points in the bankruptcy process: the date the judge approves debtor-in-possession financing, the date the disclosure statement for the restructuring transaction is approved, and the date the plan is confirmed. Figure 1 shows the regression coefficients for the Delaware and Southern District of New York venues from estimating model 2 in Table 4 using the observed pricing deviation on these important hearing dates.

As Figure 1 shows, the lower pricing deviations in the more experienced courts persist through the approval of the financing motion and disappear from the data by the time the disclosure statement is approved. This suggests the observed pricing advantage at the beginning of the process might in fact be due to the *ex ante* predictability of the destination jurisdictions—something that becomes less important as the judge issues the decisions that the market had anticipated. However, as Table OA19 shows, I find that some of the capital structure variables in Table 4 maintain their statistical significance and sign throughout the entire bankruptcy process, which suggests that I am not merely finding that the model loses all of its predictive power once a bankruptcy case draws close to the end.

As an additional check, I calculate prebankruptcy pricing deviations to see whether the market is also better at predicting the ultimate bankruptcy payoff of the New York- and Delaware-venued cohorts of firms

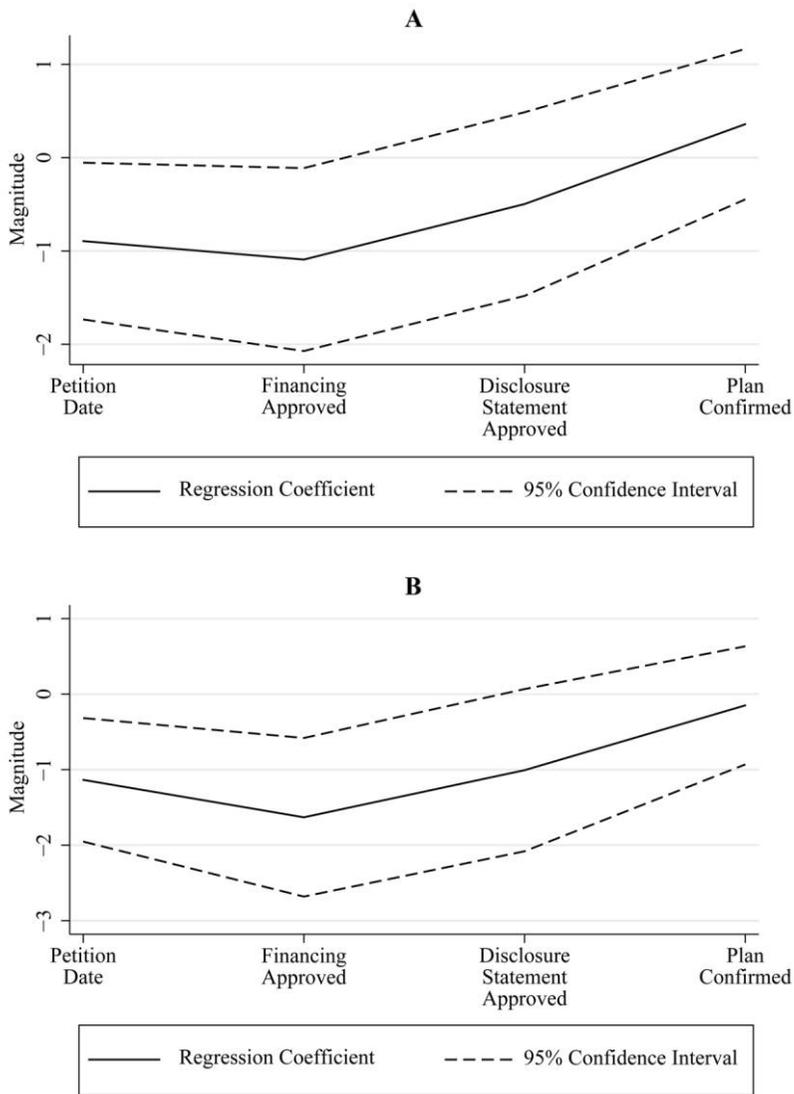


Figure 1. Pricing deviation and the (A) Delaware and (B) Southern District of New York venues.

prior to any bankruptcy filing.²⁷ Table OA20 shows that the relationship that appears at the beginning of the bankruptcy process does not appear in the data for Delaware until the petition date. On the other hand, the pricing deviation for the Southern District of New York becomes statistically significantly easier for the market to predict 30 days before bankruptcy, although the effect does not appear to be statistically significantly different from 0 earlier in the data. I hypothesize that this may be due to the market anticipating that this relatively larger cohort of firms will choose New York as their bankruptcy venue, while the market may not yet be able to predict with certainty where the eventual Delaware-venued cohort will choose to file. In any event, my failure to find a statistically significant relationship for either cohort until the eve of bankruptcy provides further comfort that these firms are not simply easier for the market to understand before bankruptcy law plays a role in the market's pricing model.

In sum, the results suggest that the market's understanding of how the bankruptcy might proceed improves after a Delaware or New York bankruptcy filing. The rich body of precedent in Delaware and New York appears to assist the market when bankruptcy law matters the most—the time between bankruptcy filing and the approval of a disclosure statement. This is consistent with the theoretical role that predictable law might play in assisting the market in pricing and is supportive of the views of bankruptcy-forum-shopping proponents.

3.2.4. Predictable Courts and Out-of-the-Money Options. Finance theory teaches that claims against Chapter 11 debtors can be thought of as European call options on the bankruptcy estate (Merton 1974). Because the typical Chapter 11 debtor is balance-sheet insolvent at filing (that is, the value of its assets is lower than the face value of its claims), junior claimants such as shareholders or unsecured creditors often find themselves

27. Caution is advised in interpreting these results for at least three reasons. First, the model uses control variables that may not have been public knowledge or may not be constant over the prebankruptcy period. Second, the model assumes that the market is pricing the claims prior to bankruptcy in terms of their discounted bankruptcy payoff, which may be a less apt choice for some firms than for others. Third, there is no way to know when the market became aware that bankruptcy was immediately imminent or what the venue would be, and there could be a systematic difference across venues or over time that is not otherwise captured by control variables. This makes the estimates for the periods closest to bankruptcy especially unreliable, because there may be unobserved heterogeneity in venue-selection knowledge—for example, the market may simply assume that the largest firms will choose the Southern District of New York.

holding the equivalent of out-of-the-money call options. If the firm's value were to increase during the bankruptcy case, it is possible that the claim would come back into the money. Junior claimants benefit from a higher level of value uncertainty, as a higher level of uncertainty implies a greater chance that junior claims will come back in the money.

Importantly, junior claimants can also engage in strategic litigation to both prolong the bankruptcy process, which also increases option value, and inflict risk and uncertainty on senior creditors to extract holdup-value settlements (Ayotte and Morrison 2009; Elias 2016). Thus, junior claimants can lose value from increased certainty.²⁸ Consequently, I hypothesize that junior claimants of Delaware- and New York-venued firms might trade at a lower value that reflects reduced option value, controlling for the ultimate bankruptcy payoff.

To test this, I study a group of deeply out-of-the-money claims with a senior claimant holding a blanket lien trading at less than 75 cents on the dollar on the petition date.²⁹ On the basis of the aggregate market value of senior claims, the median claim would require the firm's market value to increase by \$72 million for the junior claimant to come into the money, and the largest junior claimant in this subsample is more than \$473 million out of the money.

Table 6 shows the determinants of the price of the out-of-the-money claim on the petition date. In model 2, I control for firm size, whether the case was prepackaged or prenegotiated, and whether the claim is equity, and I add additional control variables in model 3. In model 4, I control for the discounted present value of the ultimate bankruptcy payoff. I also calculate the difference between the market value of all senior claims and the face amount of those claims, which proxies for how much the value of the firm would have to improve for the junior claimant to receive a distribution pursuant to the absolute-priority rule. The coefficient of this control variable takes on the expected negative relationship, which suggests that claims trade at a relatively lower value the more out of the

28. In a similar setting, Mirvis (2007) argues that shareholder class-action plaintiffs choose to file complaints in venues with less experienced courts (as opposed to Delaware's Chancery Court, which adjudicates many such disputes) because the greater variation in possible outcomes creates settlement leverage.

29. I identify my sample of out-of-the-money claims by using the price of senior claims, as the firm's prebankruptcy capital structure is endogenous and likely reflects investors' prebankruptcy assumptions about what the firm might be worth in bad states of the world. As Table OA21 shows, the results are the same if I use a deeper discount—anything up to 55 cents—as the boundary for identifying out-of-the-money claims. The results disappear as the sample becomes significantly smaller at lower boundary conditions.

Table 6. Determinants of Out-of-the-Money Prices on the Petition Date

	(1)	(2)	(3)	(4)	(5)
Delaware	-.125 (.463)	-.605* (.272)	-.586* (.272)	-.600* (.270)	-.616* (.276)
SDNY	.596 (.533)	.220 (.300)	-.164 (.270)	-.179 (.251)	-.176 (.277)
Log funded debt		-.114 (.073)	-.146* (.073)	.520** (.161)	.428* (.173)
Prepackaged bankruptcy		-.512 (.563)	-.360 (.415)	-.315 (.435)	-.430 (.531)
Pre-negotiated bankruptcy		-.126 (.241)	.125 (.191)	.072 (.191)	.056 (.206)
Equity claim		-4.099** (.221)	-2.111** (.376)	-1.867** (.342)	-1.807** (.331)
Log PV bankruptcy payoff				.369** (.063)	.381** (.063)
Log distance to money				-.626** (.146)	-.629** (.158)
Log days of bankruptcy					.140 (.111)
Log return of industry-comparable firms					-.866 (1.063)
R ²	.02	.66	.81	.81	.82
N	260	260	260	255	231
Debtors	148	148	148	146	131
Year fixed effects	No	No	Yes	Yes	Yes
Law firm fixed effects	No	No	Yes	Yes	Yes

Note. Results are from ordinary least squares regressions, with standard errors clustered at the firm level in parentheses. The dependent variable is the logged price of a junior claim that appears to be out of the money, as indicated by an observed senior claim trading at 75 cents on the dollar or less. The log PV bankruptcy payoff is the logged market value of the bankruptcy payoff, discounted to present value. The log distance to money is the logged difference between the face amount of the firm's liabilities and the market value of those financial claims on the petition date. The log days of bankruptcy are the logged days elapsed between the petition date and the date the assets leave bankruptcy court administration (the effective date of a plan). The log return of industry-comparable firms is the market-weighted return an investor would have received if she had bought the publicly traded group of stocks in the bankrupt firm's four-digit Standard Industrial Classification code on the petition date and sold it at the conclusion of the bankruptcy process.

* $p < .05$.

** $p < .01$.

money they are. In model 5, I control for the length of the bankruptcy case and the weighted return of firms in the same industry over the bankruptcy period, and the results are the same.³⁰

This analysis produces mixed results. While the Delaware dummy is persistently and negatively associated with the dependent variable once control variables are introduced, the New York venue dummy is not. I hypothesize that the raw experience level in Delaware—which receives almost twice as many large filings as New York with a smaller bench—might make a difference. Overall, the observed relationship for Delaware is consistent with the view that judicial experience might be associated with reduced option value for out-of-the-money claims.

3.2.5. Evidence of Bias toward Senior Lenders. Critics of bankruptcy forum shopping charge that regulatory competition has led judges to be overly deferential to managers and secured lenders and perhaps biased in their favor. This is a difficult claim to evaluate empirically, if for no other reason than the finding of LoPucki (2005) that some of the negative behavior associated only with Delaware and New York in the 1990s spread nationally in the early 2000s as other courts struggled to compete for cases involving large debtors. Thus, one might expect that there would be little variation associated with proxies for bias during the sample period of entrenched forum shopping. However, some data may help inform this debate.

One potential channel through which a pro-senior-creditor bias might be expressed is a transfer of value from junior claimants to senior creditors. To test whether Delaware and New York are venues for value extraction, I consider how often I observe a claimant being paid more than in full—when a debt claim is trading above 100 cents on the dollar at the end of the bankruptcy process. The absolute-priority rule says that no creditor should be paid more than in full, and if creditors are systematically being paid more than 100 cents on each dollar they are owed in a destination venue, it could be evidence of bias in favor of the secured lenders that often influence the venue decision.³¹ Table 7 displays the percentage of claims being paid more than in full at various measurement levels. The results suggest that senior creditors receive more than full repayment

30. As Table OA22 shows, results are also the same if I include a dummy variable for the cases in which management prosecuted avoidance actions.

31. Some of these observations might be false positives, as an oversecured creditor could be paid accrued interest that would look like an overpayment given the naive test statistic.

Table 7. Percentage of Debt Claims Paid More than in Full

	Delaware	Southern District of New York	Other
≥100	.09	.18	.19
≥105	.03	.08	.10
≥110	.02	.04	.05
≥115	.02	.02	.05

Note. Results are based on the observed market value at the end of the bankruptcy process.

more often in the venues with less experienced courts, but the difference between the three cohorts is slight, and, in any case, there is no evidence of a systematic pro-senior-creditor bias in the destination venues.

These results must be qualified because they rely on observed behavior. If regulatory competition causes the less experienced courts to become just as deferential and biased, we would not expect to find any observable evidence of bias in the cross section. In addition, it is very possible that some restructuring transactions favor one group of creditors over others—for example, a secured-creditor-driven fire sale (see, for example, Ayotte and Morrison 2009)—and market data may not fully reflect any value redistributed through transaction choice.³² Clearly, further research is needed on the question of deference, but an analysis of some of the data in the sample does not produce evidence of a judicial bias in favor of managers or senior creditors.

4. IMPLICATIONS AND CONCLUSION

This paper examines a cohort of firms that filed for bankruptcy in the era after Delaware and New York came to dominate the market. The results raise the possibility that the bankruptcy process in those two courts is now observably more predictable than that in other jurisdictions, which might provide important theoretical benefits to distressed firms. For example, settling important legal issues might be easier if the parties are

32. Other research finds evidence that senior creditors can use their bargaining power to pursue their own goals (for example, Gilson, Hotchkiss, and Ruback 2000; Baird and Rasmussen 2002; Ayotte and Morrison 2009; Jenkins and Smith 2014), which sometimes includes forcing inefficient liquidation, although other work argues that the ability of secured creditors to force inefficient liquidations is overstated (for example, Westbrook 2015).

able to collectively predict how the judge might rule. Lenders may feel more comfortable providing debtor-in-possession financing if they feel more confident they can predict that their rights will be upheld (Bae and Goyal 2009). Further, lawyers may find it easier to advise their clients and set expectations in a more seasoned jurisdiction.

This paper's conclusions are qualified by the limitations of the research design. The results suggest that the market appears to be able to better predict the outcome of the bankruptcy process for firms that file in Delaware and New York than for firms that file in less seasoned venues. This remains true after controlling for the firm's size, industry, law firm, changes in market conditions, and other observable characteristics. However, I cannot eliminate the possibility that these same firms would have similarly predictable bankruptcies elsewhere. One might speculate, for example, that the Chicago-based law firm that represented Tribune Media in its bankruptcy would not choose the geographic inconvenience of Delaware over the Northern District of Illinois if all else would otherwise be equal, but this research design cannot answer that question directly.

I also cannot say for certain whether transferring the 40 percent of firms that reorganize in less seasoned venues to the destination courts would make their reorganizations more predictable. It may be that the reason those firms did not file in a destination court to begin with is that their particular legal issues would not benefit from the store of precedent or expertise in those jurisdictions, and future research should investigate this possibility. Future research should continue to investigate why some firms choose destination courts and others stay closer to home.

Further, the results are more robust for Delaware than New York. I hypothesize that this might be true for two reasons. First, it may very well be that New York's cohort of firms—larger and more complex, on average—is different from the other two cohorts of firms in unobserved ways. There may be more new legal issues in megabankruptcies like General Motors and Lehman Brothers, for example, than there are in other cases. Both firms could have filed for bankruptcy in Delaware but chose New York instead, presumably for a reason that this study did not uncover. Legal precedent may be less useful for the truly unique issues that New York's larger cases bring to court. Second, there may be a difference in court cultures in Delaware and New York that make New York less predictable. Future research should compare Delaware and New York in greater detail and especially investigate the determinants of selection between the two venues.

With all of that said, the results of this paper support the view that the bankruptcy bar's forum-shopping experiment might have decreased the uncertainty associated with the bankruptcy process by speeding up the process of judicial interpretation of statutory ambiguities and building concentrated benches of experienced judges. The result of this may be, in effect, the minimization of the aggregate transaction costs created by the bankruptcy statute, a benefit that could plausibly impact the cost of capital of large firms more generally. One puzzle that arises from these results is that out-of-the-money constituencies appear to fare worse in Delaware in particular, yet one seldom observes junior constituencies using involuntary bankruptcy petitions to force companies into bankruptcy elsewhere or filing motions to transfer venue. The results suggest that the value of an out-of-the-money claim increases in the legal volatility of the court, and most large firms could be pushed into bankruptcy in inexperienced (often hometown) courts. A possible answer to this puzzle is that lender liability law and the applicable legal standard for transferring venue act as deterrents, but future research should consider why Delaware and New York have become the consensus choice when it is not obvious that all of a debtor's constituencies are best served by the plausible benefit of reduced transaction costs.

At this point, forum shopping is a well-established feature of corporate bankruptcy practice. In other contexts, researchers have used the concept of network effects to show how the market's initial preference for a product creates a lock-in effect as the preferred product accrues benefits that increase its value. For example, judicial precedent and expertise in Delaware business law make it difficult for other states to compete for corporate incorporations because those benefits are not easily duplicated (Klausner 1995). Future research should investigate whether a similar process might have happened with bankruptcy courts and whether the deference concerns raised by critics might be less pressing in an era in which the judges in destination venues have more bargaining power than they had in an earlier period. Once a jurisdiction builds up advantages over time, alternatives may not be reasonable substitutes even if they would have been in a counterfactual world without extensive forum shopping.

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